WEDNESDAY, MARCH 31st, 1897.

The Twenty-Third Annual General Meeting of the Society was held in the Linnean Hall, Ithaca Road, Elizabeth Bay, on Wednesday evening, March 31st, 1897.

The President, Mr. Henry Deane, M A., M. Inst. C.E., F.L.S., in the Chair.

The Minutes of the previous Annual General Meeting were read and confirmed.

The President then delivered the Annual Address.

PRESIDENT'S ADDRESS.

I have the honour once more to address you from this Chair.

The year just concluded has been one of fair activity, and the papers read before the Society have been of an important character. There have been nine ordinary meetings, and at these forty-four papers have been read.

Some of the papers have had to me a particular interest as bearing on one of the subjects which I took up for special treatment in my Address last year. These are as follows:—Captain Hutton communicated a paper on the probability of a former land connection between Australia and South America. Mr. Ogilby presented some observations on groups of fishes the distribution of which can scarcely be understood except on the supposition of a former Antarctic continent. Professor David has contributed valuable information on the occurrence of diatomaceous earth and Radiolaria, and the Rev. J. M. Curran read some notes, which are, as I understand and hope, preliminary to a paper, on the supposed glaciation of Mt. Kosciusko.

Mr. Maiden and I have been working at Eucalypts and have presented a contribution on the subject. It is one that has

already been dealt with by such competent authorities as the late Baron von Mueller, Mr. Howitt, and the late Rev. Dr. Woolls; but many species have at present been considered only from a Victorian standpoint, and demand investigation as to habits and variation when found in New South Wales.

The difficulty of defining what is a species among Eucalypts or indeed in any large and variable genus is very great. It is very easy to make very serious mistakes by grouping some that ought to be kept separate, or in the case of very variable forms, giving specific rank to mere varieties.

A curious example of errors that may be committed before sound knowledge is acquired I find in a Report to the Lieutenant Governor by Mr. William Swainson, F.R.S., in 1853. This gentleman divided up what he called the "Eucalyptidæ" into seven genera and 1520 species and varieties, while of the genus *Casuarina* he found 213 species, some of which he was obliged to leave unnamed, having exhausted his vocabulary. The most difficult species of *Eucalyptus* are probably those with smallish fruits, for there is then so little opportunity to seize hold of distinguishing characters, and it is only by taking into account forms of buds, anthers, fruits, leaves, seedling and mature, bark and perhaps the wood itself that anything like certainty can be arrived at.

What an opportunity is here for some of the spare energy of the old country which spends itself on monographs of a small and variable genus of Compositæ! What scope of useful investigation exists in the study of the variation of vegetable forms on a large continent like our own, which has been altogether free from the destructive and thinning out action of Glacial Periods and catastrophes, and where opportunities of almost unlimited variation exist!

During the year four ordinary Members and one Associate have been added to the Roll, and one Member has resigned.

The Society has lost the services of Mr. U. A. Henn and Dr. Martin on the Council, but I am glad to be in a position to say that they will continue their Membership although circumstances

have induced them to take up their residence outside New South Wales. Messrs. Brazier and Whitelegge have also resigned from the Council.

We have to deplore the loss of our oldest Honorary Member, Baron F. von Mueller, who was elected on the 22nd January, 1876. To this event I shall take the opportunity of referring presently.

The distinguished Algologist, Professor G. B. Toni, of Padua, has been elected an Honorary Member of the Society.

In accordance with the resolution passed at the beginning of last year, a sound investment having been found for the funds left by the late Sir William Macleay, the Council took steps to invite applications in England and the Colony for the position of Macleay Bacteriologist. Five applications were received, but after considering the qualifications of the applicants, the Council has decided not to appoint any of them, but to give a wider publicity to the Society's requirements and advertise afresh later on with a view to obtaining a better selection. In the meantime, the principal will be increased by the year's interest, so that pecuniarily the delay will not be a loss.

Baron F. von Mueller.

I must now take the opportunity of saying a few words in tribute of respect to the memory of the late Baron F. von Mueller, whose friendship and good qualities many of us learnt to appreciate.

I do not intend to offer a lengthy account of the Baron's life, as that has already been done by others far more fitted to the task than myself. I may refer to the interesting account given in the "Sydney Mail" of the 17th October last, written some time ago by the late Rev. Dr. Woolls, and to that published in the "Victorian Naturalist," (No. 7, Vol. xiii.), which is due to the able and sympathetic pen of Professor Baldwin Spencer.

Baron F. von Mueller is a fit compeer of such men as Robert Brown, Dr. Hooker, and Mr. Bentham. He was a man of indomitable energy and perseverance, and during his 44 years of official life he achieved such results as few can boast of.

His reputation was a world-wide one, and there are few countries in which he has not at some time or other found correspondents.

Baron F. von Mueller arrived in Adelaide in 1847 and immediately set himself to prosecute his favourite study of botany. In 1852 he was appointed Government Botanist of Victoria, and was thus enabled to commence his investigations on the flora of a part of Australia which was untouched by Robert Brown. At that time he commenced a series of most arduous journeys to the Australian Alps and elsewhere, often unattended; and what that meant in those days can be imagined only by those older residents of this country now living who have had experience of the inhospitable character of the Australian bush and the dangers connected with it. In 1855, one of the most important journeys was made; he then accompanied Mr. Gregory to the north and north-west of Australia, and the expedition undoubtedly until the time of the recent Horn Expedition stood out above all others for its valuable scientific results; and in general interest and importance of discovery it was second only to Leichhardt's.

In the earlier part of his career Baron F. von Mueller was much in the field and had opportunities of studying the forms and habits of living plants which later in life he missed.

Included in the vast collections which enabled Bentham to carry out that unique work, the "Flora Australiensis," the only complete continental Flora written, were more than fifty "large cases" of specimens collected or forwarded by the Baron to Kew; and to his assistance was the success of the work largely due. There are now more than double the species of vascular plants described compared with those known to Robert Brown. Mr. Bentham in his eulogy on Robert Brown (Proceedings of the Linnean Society of London, May 24th, 1888), gives 4,200 as his total. In the "Flora Australiensis" are described 7,000 species. The second edition of the "Census" published in 1889 includes 8,839 species, distributed among 1,409 genera and 156 orders.

It was to be expected that a large amount of work would remain unfinished. The complete investigation of the flora of a continent is a work not of one generation nor of two, but the

foundation has been laid for the carrying on of the study of various important groups, and among the most interesting of the subjects to which the Baron devoted his attention are those of genera and orders possessing in Australia peculiar characters and forming often a special feature of the flora. I refer in particular to his Monograph entitled "Eucalyptographia," consisting of descriptions, with plates, of 100 species of the genus Eucalyptus, and to the series of illustrations of Acacia, consisting of 13 decades or 130 species, Salsolaceæ of 9 decades or 90 species, and of Candolleaceæ 1 decade only. A work on the Myoporineæ containing figures of a large number of the species of *Myoporum* and *Eremophila* was also begun and one volume completed. When it is considered that there are probably at least 150 species of *Eucalyptus* and that only 100 are given in the "Eucalyptographia," and that out of more than 300 species of the genus Acacia only 130 are figured, it will be seen that a large amount of work remains to be done with those groups alone.

The Baron's note on *Boronia floribunda*, read at the meeting of this Society on September 30th last, is believed to be his last scientific contribution.

A fitting memorial to the late Baron would be the publication of a supplemental volume to the "Flora Australiensis." As he took so important a part in furnishing material for the seven existing volumes, it would be a graceful tribute to his memory to dedicate the supplement to him. This work should of course be carried out on the lines and according to the same system as that adopted in the "Flora," which, whatever its objections may be, has very much to recommend it, not only on account of its being that made use of in the "Genera Plantarum," but chiefly because a supplement could only thus be of real utility. It would, however, be a convenience if at the end of the volume a reference in tabular form to the system and nomenclature of the Baron's Census were supplied. It is to be hoped that in whatever way the work may be carried out, all jealousies will be laid aside and the greatness of the man to whose memory the tribute is offered alone remembered. This volume might well

be made a joint work subscribed for by the four Governments of South Australia, Victoria, New South Wales and Queensland, and jointly edited by the four representative botanists of those colonies.

I should now like to add a few words on the subject of nomenclature, but I do not wish that these remarks should be taken in any way as disparaging to the late Baron's work. Every man has a right to his own views, and certainly none more so than the late leading botanist of Australasia, but there are few who agreed with him on certain points, and some who have followed his methods during his lifetime will probably feel themselves justified in now throwing off the restraint previously imposed upon them.

Many of the well known names of the "Flora Australiensis" were dropped by the Baron and do not appear in their expected places in his "Census of Australian Plants." Other names which he considered to have the right of priority have instead been adopted by him, to the great discomfort of most of us. Under one large genus, many generic names with which we are familiar have been grouped For example. such genera as *Crowea*, *Phebalium*, *Asterolasia* and many others are thrown into *Eriostemon;* and *Astroloma*, *Leucopogon*, *Melichrus*, *Acrotriche*, *Monotoca*, *Lissanthe* and a host of others are suppressed and the species placed under *Styphelia*. The annoyance is great enough when in looking a plant up you miss its generic designation, but if, as in the case of many, you lose the specific name as well, it is confusion worse confounded. Priority should not be the only guide when adopting a name, but use must be taken into consideration. Mr. Thistleton Dyer in his Address to Section K of the British Association for the Advancement of Science, 1895, says that to him "botanists who waste their time over priority are like boys who when sent on an errand spend their time in playing by the roadside. By such men even Linnæus is not to be allowed to decide his own names." And in another part of the same address he makes the pertinent remark that "if science is to keep in touch with human affairs, stability in nomenclature is a thing not merely to aim at but to respect. Changes become necessary, but should never be insisted

on without grave and solid reason;" and in a note he calls attention to Darwin's saying, "I cannot yet bring myself to reject any *well known* names." No doubt the Baron thought he had grave and solid reason to change some names, and we should be loth to charge him with loitering on his errand like the schoolboy, but I am sure all of us prefer the names we became used to through the "Flora Australiensis"; let us therefore adhere to them as much as possible.

Mr. R. D. Fitzgerald's "Australian Orchids" consisted at his death of one Volume of seven parts, and four other parts towards a second Volume. One hundred and eighty-three species were figured and described, with interesting notes on their habits and modes of fertilisation by Mr. Fitzgerald himself. Seeing the number of fine drawings still unused, it was proposed to continue the publication. The assistance of Mr. A. J. Stopps was secured for the lithographic work, and I was asked to work up the text. Many friends came forward to help with information, and Part 5 of the second Volume was brought out under the editorship of Dr James Norton in 1895. About half the plates required for Part 6 and some notes for the text are ready, but there is no money to go on with the publication. Only a small sum is really necessary to complete this part, but the Government steadfastly refused last year to place any money for the purpose on the Estimates. It will be a great pity if this part cannot be finished, and also Part 7, which would make up the second Volume. I hope a renewed effort may be made some day to induce the Government to provide the requisite funds for carrying out this essentially Australian object.

One of the scientific events chronicled for the past year is the ineffectual attempt to execute a wish of Charles Darwin to pierce a coral island to its foundation and, by bringing up a core, test the mystery of its origin. A committee appointed by the Royal Society of London for the purpose of this investigation had a man-of-war placed at their disposal by the Admiralty. The New South Wales Government further assisted them with a loan

of boring machinery. The expedition, of which Professor Sollas was placed in charge, sailed from Sydney in May, to Funafuti, an atoll lying half way between Fiji and the equator which had been selected as the scene of operations.

The impression usually prevailing that a coral reef was a dense and homogeneous mass was soon dispelled when the diamond drill began to work, and it was shown instead to contain caves and fissures filled with quicksand. The latter proved too much for the apparatus at command, and after penetrating to a depth of 105 feet in the first instance and 72 feet in the second, further attempts were abandoned.

Although the chief line of inquiry was defeated, important results were achieved by the officers of H.M.S. Penguin, whose soundings perfectly develop the submarine slope and contour lines.

These observations and some made later in the year by officers of the same vessel on the Alexa Bank throw much light on the conditions under which coral formations appear to take their rise, which are apparently not those assumed by Darwin.

The naturalists attached to the Funafuti expedition amassed collections illustrative of the ethnology, zoology and botany of the island, and a memoir based on the gatherings and observations of Mr. Hedley, who was attached to the expedition, is now in course of publication by the Australian Museum. The reports so far reflect immensely to the credit of Mr. Hedley, who seems to have allowed no detail, however apparently unimportant, to elude his observation. The general account of the atoll and of the manners and customs of the inhabitants, written by himself, will be read with interest by all, and the hope will naturally arise that Mr. Hedley may have opportunities accorded to him of taking part in future investigations of the kind.

A reprint of Professor Sollas's Report, published in "Nature," of February 18th, came to hand yesterday.

Another expedition for the investigation of coral reefs—I refer to Professor Agassiz's visit last winter to the Great Barrier Reef had, owing to bad weather, which is a most unusual occurrence

at the time of year, to be given up before its main object had been attained.

News has just been received that another scientific excursion to the Pacific has met with some success. After enduring considerable toil, hardship and danger, Dr. Willey has, in the Loyalty Islands, succeeded in obtaining eggs of the Nautilus, but unfortunately these have failed to develop.

A remarkable discovery in morphological botany has recently been made in Japan of another connecting link between flowering and flowerless plants. The discoverers are Professor Ikeno and Dr. Hirase, who have found in *Cycas* and *Ginkgo* the fertilisation of the ovule effected by a partial penetration of pollen tubes, and a subsequent development of antherozoids for the completion of the process.

With regret we learn from "Nature," of February, 18th, that the veteran palæontologist and botanist, Baron Constantine von Ettingshausen, had died at Graz at the age of 71.

Horn Expedition.

In my Address of last year lengthy reference was made to the first instalment of the "Report of the Horn Scientific Expedition to Central Australia"—Part ii. Zoology, then just published. Three additional parts—Part i. Introduction, Narrative, Summary, &c., with Map, by Professor Baldwin Spencer, M.A.; Part iii. Geology and Botany, by Professor Tate, and J. A. Watt, M.A., B.Sc.; and Part iv. Anthropology, by Professor Stirling and Mr. Gillen—have since been issued under the able editorship of Professor Spencer, completing this important work. The Report in its complete form, as a contribution to Australian scientific literature, has fully justified our expectations of its importance, and it demands a further expression of our indebtedness to Mr. Horn, the promoter, and to all who have shared in its production.

A very substantial increase of knowledge in all departments has been gained, but Professor Spencer has so ably summarised the results that it is needless to attempt a re-summary. I will merely refer to his remarks on the relations of the Autochthonian

Flora to the assumed early "Cosmopolitan Flora" on pages 174 and 175, in which he points out that the autochthonian flora of the west was, from very early times, to a large extent shut off by barriers from an immigration of other types, and that it is difficult to see how, if the autochthonian has been derived from the cosmopolitan, representatives of typical Australian genera only are found, and not a trace of such doubtful forms as *Quercus*, *Betula*, *Salix*, &c., upon the presence of which in fossil remains the theory of the cosmopolitan flora in Australia really rests.

Professor Spencer's "Narrative" is of special interest. We have many narratives of Australian travel and exploration, but these have been written by the leaders of expeditions, much of whose time and attention was necessarily devoted to administrative details, and absorbed by the anxiety unavoidably connected with these; but we have here a narrative from the pen of an expert biologist, well versed in the subject of the natural history of Australia, with a keen eye and a ready pencil, and pursuing his work undistracted by drawbacks such as those alluded to above. And the work is rendered additionally attractive by an admirable series of topographical and other views reproduced from actual photographs. Nature was unfortunately in a very dry mood, and the opportunity of witnessing the advent of good rains, and the circumstances attendant on a Central Australian flood, did not present themselves. Floods and droughts have, however, to be taken as they come.

The experiences of the expedition have afforded Professor Spencer opportunity for a masterly exposition of some of the probable former relations of Australia, and a comparison of the special features of its botanical and zoological subdivisions, resulting in the conclusion that these are not coincident. The names applied to his Subregions—Torresian, Eyrian and Bassian —are, I think, particularly happy, as avoiding all objections of implied theory and dogmatism.

Professor Spencer, in discussing the question of the route by which Australia received its mammals of characteristic types,

decides in favour of a former land connection between South-eastern Australia and South America, through what is now Tasmania, and thus adds his support to a theory, the objections to which are continually losing weight.

In my Address last year I pointed to the necessity of this connection in former times in order to account for the affinities of a portion of the floras of Australia, New Zealand and South America, and the occurrence in a fossil state in South America of marsupials allied to our own. The chief objections are—first, that an ocean of considerable depth lies between these countries, the bottom of which, it is therefore supposed, could never have been above the surface. As a matter of fact, even if Wallace's 1000 fathom limit of possible elevation or depression could be acknowledged, it is to be remarked that not enough soundings have been taken in the higher latitudes to prove the non-existence of submerged plateaux. The lowest continuous line of soundings seems to have been made by the officers of the Challenger; it lies near latitude 50°, and there is to the south of that parallel plenty of room for extensive plateaux to show themselves or even quite shallow depths when soundings are taken. The other objection, that the temperature and climate would have been too severe, can scarcely have weight. In the early and middle Tertiary mild temperatures existed in the northern hemisphere up to latitude 79° in Spitzbergen, and 81¾° in Grinell Land, and there is no reason why, at the same epochs, if the disposition of the land was suitable, there should not have been temperatures favourable to life in the corresponding latitudes near the south pole. Fossil remains from the Straits of Magellan indicate tropical conditions. During the Pliocene, temperature generally became lowered, and the vegetation of the temperate zone had begun to retreat from the North Pole; but even if the same process took place at the South Pole, there might still be abundant warmth between, say, 55° and 70°, to permit of the existence of a luxuriant vegetation and fauna.

I have been unable through lack of time to carry out my intention of completing the comparison of the tertiary fossil leaves with those of the existing vegetation. Sufficient has, however, been done with the assistance of Mr. R. T. Baker, F.L.S., to confirm me in the opinion I last year expressed, that the fossil flora would find its representatives in the existing coast vegetation. Some of the fossil fruits of the Pliocene Gold Leads also closely resemble those of to-day on the coast, but as structure has been almost entirely lost, there is not that certainty that one would like to find. It seems, however, quite clear that before seeking for analogies in distant countries careful comparisons with the existing flora should be made, and this is not the method that the eminent palæontologist, in whose hands the fossil plant remains from Dalton, Vegetable Creek and Oxley were placed, adopted. Taking into consideration the difference between the Eocene and Miocene climate and that of the present period, we might expect to find existing types a few degrees further south in the fossil state, but that is quite a different idea from going to the other side of the earth for analogies.

I can find little or no information about the fossil tertiary floras of Western Australia, South Africa and South America. This is much wanted, as also further information about fossil remains of the tertiary beds of Kerguelen Island.

Some months ago, when on a visit to South Gippsland, Mr. J. H Wright took me to some leaf beds lying on a horizon above the "Lower Basalt." The most interesting finds on that occasion were leaves in all respects resembling those of a species of *Coprosma*, "domatia" and all. This is a curious indication of the antiquity of these peculiar structures. *Eucalyptus* was not noticeable in these beds, and the vegetation appeared to have such a character as would imply humid atmospheric conditions. Mr. Wright showed me some beautiful leaf remains from the siliceous shales below the "Lower Basalt," in which there were a good many leaves of the *Fagus* type, as well as what might be taken for an early form of Eucalypt.

Through the kindness of Mr. R. L. Jack, Government Geologist of Queensland, I have received a number of samples from the Oxley beds, referred to in my Address of last year. The impressions are very fragmentary, and thus very difficult to make out. They seem to me as a whole to be rather conspicuous for the scarcity of Eucalypts and Proteads as we know them, a circumstance which, as I have already indicated, we need not be at all surprised at.

Affinities of the South African Flora.

The belief in the former connection between Australia and South America is continually obtaining more adherents, but the possibility of a land bridge having ever existed between South Africa and Western Australia is treated with much greater incredulity. The affinities of the existing floras, however, seem to point to it as the only possible explanation. Strong evidence of a connection in the Carboniferous Period has already been adduced by Dr. Blandford and others, on the ground of a common flora, which flourished not only in South Africa and Australia, but in Southern India and South America as well.

Had we not this evidence from Carboniferous times, we must acknowledge that the resemblance between the existing floras of the south-west region of South Africa and that of Australia, and particularly of Western Australia, is too remarkable to be accounted for by saying that they are relics of a once cosmopolitan flora, and that their peculiarities have been produced by the selective action of the floral climates. Those botanists who have closely studied them would not be contented with any other explanation than that of actual land connection, or at least of a former tolerably close proximity of the land areas, after the peculiarities of the flora had become developed. Strips of deep sea now separate the two countries, but it does not follow that there was never any land bridge between them. It is certain that parts of the ocean where now there are depths of 1500 fathoms have been land in the Miocene—for example, that from New Zealand northwards. Could we not allow of a local sub-

sidence of, say, 2500 fathoms since the Cretaceous? That is all that is necessary.

Last year I referred to the comparison made by Dr. Hooker in his Introduction to the "Flora of Tasmania." The following additional particulars from Dr. Harry Bolus's article in the "Cape Handbook" will be of interest.

The region over which the Proteaceæ are found, and to which they are practically confined, is the south-west region. It is a narrow strip about 400 miles long, extending from and including Cape Town to Port Elizabeth, when it gradually but rapidly merges into the tropical African region. The vegetation of this latter region, like the luxuriant vegetation on our own east coast, extends southwards from the tropics far into temperate latitudes. The width of the south-west African region is about 50 or 60 miles on the average, and its northern boundary is a very sharply defined one. To the north is the Karroo region, a particularly remarkable one also as will be seen. The flora of the south-west region is characterised by abundance of Rutaceæ, Bruniaceæ, Ericaceæ, Proteaceæ, Restiaceæ, Leguminosæ, and some others. The Karroo region which adjoins it on the north is noted for the complete absence of the orders named, and for the scarcity of Leguminosæ. The other regions of South Africa mentioned by Mr. Bolus are the Composite and the Kalahari, but these do not interest us to the same extent.

South Africa is, in Mr. Bolus's paper, assumed to be limited by the Tropic of Capricorn. It exhibits a most remarkable variety of plant life, and a comparison with Australia presents some remarkable analogies:—

Australia contains 152 orders and 1300 genera.
S. Africa „ 142 „ „ 1255 „

In Australia there are 520 endemic genera.
S. Africa „ 446 „ „

But it is to be noticed that the area of Australia is five times that of South Africa, and it extends northwards to 10° south latitude, instead of being limited by the Tropic of Capricorn.

The south-west region possesses the following orders in the greatest abundance :—

1. Compositæ.
2. Leguminosæ.
3. Ericaceæ.
4. Proteaceæ.
5. Irideæ.
6. Geraniaceæ.
8. Cyperaceæ.
9. Restiaceæ.
10. Liliaceæ.
11. Orchideæ.
12. Rutaceæ.
13. Scrophularineæ.

A comparison with the most abundant Australian orders shows that *Irideæ*, *Geraniaceæ*, *Restiaceæ*, *Liliaceæ*, *Rutaceæ*, and *Scrophularineæ*, although existing, are not so prominent, and would have to take a lower place, and the orders *Myrtaceæ* and *Goodenovieæ* would be substituted. The order *Ericaceæ* is represented by the closely allied order *Epacrideæ*.

With regard to the other orders, it is to be noticed that *Restiaceæ*, although not so abundant, are peculiarly Australian ; that the suborder *Boronieæ* of *Rutaceæ* is peculiarly Australian, like the *Diosmeæ* of the same order in South Africa ; and that among *Liliaceæ* there is a peculiar genus—*Nanolirion*—which is closely allied to *Herpolirion* of Australia, Tasmania and New Zealand.

The study of geological phenomena and the distribution of life on the earth lead to two important conclusions: *first*, that the earth's surface has been subject to repeated and extensive deformation, implying a considerable amount of flexibility of the earth's crust, whereby the land connections have been varied at different times; and *secondly*, that over portions of the earth's surface extraordinary changes of climate have taken place, so much so that glacial and temperate, subtropical and even tropical conditions appear to have become interchanged.

Permanence of Ocean Basins.

In spite of the undoubted truth of the first of the above propositions, the theory of the permanence of ocean basins and continental areas holds still a very strong position in the minds

of many. The chief argument in its favour lies in the supposed absence of deep sea deposits on dry land.

Speaking on this subject, Professor H. Alleyne Nicholson in his Presidential Address to the Royal Physical Society of Edinburgh, 1894, points out that the deepest deposits are necessarily thin, scanty and of limited area. Radiolarian deposits, which are supposed to indicate deep sea, have been discovered of various ages. In Lanarkshire they are accompanied by green and red mudstone, a forcible reminder of modern deep sea deposits.

Professor David's observations tend to shew that radiolarian deposits do not necessarily indicate deep sea. Probably in this case we should have to judge by the circumstances under which the Radiolaria are found, and it is to be remembered that land drifts and vegetable débris may be found mixed with deep sea deposits in the most incongruous manner. The dredging operations between the west coast of Central America and the Galapagos carried out between February and May, 1891, with the U.S. Fish Commission steamer Albatross, under charge of Alexander Agassiz,* showed together with characteristic globigerina ooze, a large amount of decayed vegetable matter. Terrigenous material was dredged up from depths of over 2,000 fathoms, and with it logs, branches, twigs, and decayed vegetable matter. Off the West Indies immense quantities of vegetable matter had also been obtained from depths of over 1,500 fathoms. It is evident that if such materials were found fossilised in such surroundings they would be thought to indicate shallow depths.

Professor Poulton in his Address to the Zoological Section of the British Association last year, refers to the results of some of the Challenger dredgings from great depths as follows :— "These most interesting facts prove furthermore that the great ocean basins and continental areas have occupied the same relative positions since the formation of the first stratified rocks, for no oceanic deposits are found anywhere in the latter." This

* Bulletin Museum Comparative Zoology, Harvard College, Vol. xxiii. p. 12.

is curiously at variance with what Mr. Marr was saying at or about the same hour of the same day in a neighbouring hall. Mr. Poulton's statement is an expression of the theory of the permanence of ocean basins and continental areas, and it is that neither more nor less. It is desirable therefore to inquire what is meant. As it stands, it is a general statement too vaguely put to be of much use. Does it mean that the whole of the great ocean basins and the whole of the continental areas have always occupied the same relative positions? Clearly not, for we know that nearly or quite all existing land has at some time or other been under the water, and there have been land connections where there is now sea. The proposition must then be reduced to this, that *portions* of the great ocean basins and *portions* of the continental areas have occupied the same relative position. In other words, some portion or other of the great ocean basins has always been under the water and that some portion or other of the existing continental areas has always been above the sea. The statement thus corrected is useless to us; it affords no explanation of the distribution of life on the earth, for it may be true that some areas of existing land and water have always been land and water respectively, and yet we know that continental areas have been differently divided and cut up, and the same is the case with the seas. If it was intended to mean that the continents and oceans had been practically the same through all time as they are now, it is incorrect. For example, we have very good reason from the study of the flora to believe that in Permo-Carboniferous times South Africa, Southern India, Australia and South America formed part of one continent, and that in the early Tertiary Period North and South America were broken up into quite distinct land masses, and that in the same period and earlier Europe and Western Asia were indented and crossed by seas in a way that would make that part of the world quite unrecognisable now.

Mr. J. E. Marr in his opening address to Section C (Geology) of the British Association, 1896, says :—"We have been told that our continents and ocean basins have been to a great extent

permanent as regards position through long geological ages; we now reply by pointing to deep sea sediments of nearly all geological periods, which have been uplifted from the ocean-abysses to form portions of our continents; and as the result of study of the distribution of fossil organisms we can point almost as confidently to the sites of old continents now sunk down into the ocean depths. It seems clear that our knowledge of the causes of earth movements is still in its infancy and that we must be content to await awhile until we have further information at our disposal."

Captain Hutton says:—"We know as a matter of fact that continental areas are liable to subsidence, and that oceanic areas are liable to elevation; and we cannot as yet place a limit on the possible amount of continental depression or of oceanic elevation."* Further on (p. 411) he says:—

"We certainly do find a large number of geological periods represented in Europe, Asia, America, Australia, and New Zealand, but in all cases there are also long periods unrepresented, especially in the Palæozoic era, when there are many physical breaks in continuity, accompanied by an almost complete change in animal life, and Sir A. Ramsay says that these breaks may each indicate a period of time as great as the vast accumulations of the whole Silurian series. The question is, What was the condition of these areas during the unrepresented periods? Certainly they might have been land, but also they might, in some cases at least, have been deep ocean."

Rigidity of the Earth.

As the facts of the deformation of the earth's surface as well as that of alterations of climate depend largely upon the flexibility of the crust, it will be interesting to consider shortly the conclusions that have been arrived at as to the rigidity of the earth.

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* "Has the Deep Ocean ever been Land?" New Zealand Journal of Science, Vol. i. p. 410.

Lord Kelvin and Mr. G. H. Darwin, from a study of the long period oceanic tides, conclude that the earth's mass as a whole is more rigid than steel but not quite so rigid as glass. Such a degree of rigidity would at first sight appear to preclude any alteration of the levels of the land with respect to the ocean; we know, however, that certain tracts of the earth's surface are rising and others falling, so that the question arises what such an amount of rigidity implies.

Mr. R. L. Woodward in a paper entitled "The Mathematical Theories of the Earth," published in the American Journal of Science, Vol. 138, p. 343, says:—"Whatever may have been the antecedent condition of the earth's mass, the conclusion seems unavoidable that at no great depth the pressure is sufficient to break down the structural characteristics of all known substances and hence to produce viscous flow whenever and wherever the stress difference exceeds a certain limit, which cannot be large in comparison with the pressure." Internal fluidity is therefore not a necessary condition to account for movements of the crust.

Roche considered that geological phenomena were best explained by postulating a solid nucleus with a zone of fusion separating the crust from the nucleus.

In a paper entitled "An elementary proof of the earth's rigidity," published in the American Journal of Science, Vol. 139, p. 336, the author, Mr. George F. Becker, points out that although the earth is a very rigid body, it does not necessarily follow that it is solid. The assumption of solidity is objected to by geologists as opposed to the possibility of the occurrence of geological phenomena. There is, however, no conflict between geology and physics. He says:—"Time enters into the expression of viscosity, and the fact that the earth behaves as a rigid mass to a force which changes its direction by 360° in 24 hours is not inconsistent with great plasticity under the action of small forces which maintain their direction for ages. For a considerable number of years I have constantly had the theory of the earth's solidity in mind while making field observations on upheaval and

subsidence, with the result that, to my thinking, the phenomena are capable of much more satisfactory explanation on a solid globe than on an encrusted fluid one."

Changes of Climate.

The changes of climate, which occurred in the Carboniferous period, if the phenomena are rightly interpreted, are much more extraordinary than those of the Pleistocene when the so-called Glacial period or periods set in, for the latter appear to have been chiefly due to a general cooling of the poles and a consequent enlargement of the ice caps. The latter phenomena are visible both in the northern and southern hemispheres, whereas the glacial action which appears to be traceable in the Carboniferous period extended over Southern India, South Africa, Australia and South America only. At this time Dr. W. T. Blandford (Part 2, Vol. xxix. of the Records of the Geological Society of India) says that these countries formed a continent, judging from the peculiar flora which characterises them. In each case a boulder bed "undoubtedly glacial in origin" has been found associated with them. Dr. Feistmantel states that the *Lepidendron* flora was swept away at the ushering in of the new conditions and gave way to the *Glossopteris* and *Gangamopteris* flora. He shows that a shifting of the pole would not account for the new conditions, as on the opposite side of the earth the vegetation remained unaffected, and the difficulty of imagining so large an area of the earth's surface influenced by the advance of the polar cap is all the greater seeing that since the writing of Dr. Feistmantel's report South America has been added to the territory. Dr. Blandford points out how this area must have been cut off from the rest of the world by sea, so that once the vegetation had changed it was preserved from immigration of the old flora; but how did it become changed? could it have been by cold, seeing that the other side of the earth was unaffected? The phenomena of stranded boulders, groovings and scratchings are extraordinarily like what glaciation produces, but can they only be accounted for by ice? Assuming the glacial phenomena to have existed in

certain districts only over this large area, can these local conditions be considered to have been sufficient to produce a complete change in the flora? Mr. Dubois in "The Climates of the Geological Past," attributes the alteration to a general raising of the land, but it still seems rather strange that all the land should be raised, and although coal was still formed, no suitable positions should be left for the old flora. He says:—"Just as during the Carboniferous Age an extensive lowland, cut up by the sea into a large marshy archipelago, accounts for the formation of coal over nearly the whole of the northern hemisphere, to such an extent that comparison can only be made with the extensive deposits of Jurassic coal, extending from Western Asia to Australia, it seems that a large mountainous continent ("Gondwána Land" of Suess), at the south of the equator, has caused extensive accumulations of ice in suitable places. A great uniformity of orographic conditions over extensive continental parts of the earth's crust seems to have been characteristic of the Coal period. It is thus possible, and even probable, that by a gradual upheaval of such a continent, the changed conditions of existence caused the development of a new flora, which only much later, in the beginning of the Mesozoic period, should find in Europe, in the higher upheaval of the ground, conditions it was better fitted for than was the older Palæozoic flora which in consequence would suffer extermination. Traces of glaciation are believed to have been actually found in the Permian formation of Europe. From those high centres of acclimatisation the new flora, accommodating itself to a higher temperature, could then have gradually spread over the lowlands."

Up to quite recently there were, and perhaps even at the present time, there are geologists who hold that the Glossopteris Flora belongs to a much later period of the world's history than the Lepidodendron Flora of the Coal Measures; but representatives of the two floras have been found associated in the same beds, which must be accepted as a final and conclusive proof of their contemporaneous existence. (Rec. Geol. Sur. of India, Vol. xxix. Part 2, p. 58).

Glacial phenomena are reported from various ages between the Carboniferous and Pleistocene Formations, and the phenomena as exhibited in Australia are well set forth in Professor David's Address to Section C. of the Aust. Assoc. Brisbane, 1895.

The most important and tangible of the recorded phenomena in the northern hemisphere are those of the "Great Ice Age," as it is called, in the title of Dr. James Geikie's book. The temperature of the earth, the north pole at least, was cooling down at the end of the Tertiary Period, and the cold culminated in the Pleistocene. Dr. Geikie says that at least six different periods can be proved during which the cold advanced and retreated, and between which mild conditions prevailed. Other geologists count these to be less in number.

Various explanations have been given for the spread of Arctic conditions from the pole, the most noted being probably that known as Croll's theory. Dr. Croll argues that the orbit of the earth, in consequence of the varying positions and attractions of the planets, increases in eccentricity at long intervals of many hundreds of thousands of years. The Glacial epoch occurred in one of these periods. High eccentricity would, when the axis of the earth was inclined in the line of the major axis of the orbit, cause long mild summers and short winters in one hemisphere and short summers and long cold winters in the other. Under the latter conditions, great cold and accumulation of snow and ice and what is called a glacial period would result. With the precession of the equinoxes, the conditions would alternate in the northern and southern hemispheres till the orbit of the earth lost its extreme eccentricity.

Major-General Drayson considers that the pole describes a circle round a point 6° from the pole of the ecliptic, so that, 13,700 B.C., the angular distance of the two poles would be such as to bring England within the Arctic circle.

"Professor G. H. Darwin has considered the possibility of the pole having worked its way in a devious course 10° to 15° from its present geographical position, but points out that such a movement would require extensive surface deformation and shift-

ing of surface weights not easy to understand." (Great Ice Age, Note p. 791).

Sir Charles Lyell considered that all climatic changes could be explained by gradual changes in the distribution of land and water. There are few that now hold this view. It is to be remarked that in Pleistocene times the distribution of land and water was practically the same as now, and yet it was just in that period that the most remarkable oscillations of temperature conditions occur.

Dr. Geikie in the work already referred to points out that there are oscillations of temperature and rainfall shown by advance and retreat of glaciers, rising and falling of level of lakes and inland seas, and asks whether these may not be due to cosmic causes, and whether such causes may not have to do with the larger and more extensive oscillations producing glaciation or mild temperatures up to near the pole.

As regards the question of the geographical shifting of the pole, I find in "Nature," of September 25, 1884, a letter by Mr. Flinders Petrie referring to an Address by Professor Young, which stated that a change of one second per century had been noted at Pulkowa in the earth's axis. Other corroborations of the same fact exist. He says:—"Such a change might be effected by causes which are beyond our observation; as, for instance, unbalanced ocean circulation equal to a ring of water only 4 square miles in section moving at a mile an hour across the poles." Mr. Petrie refers to the Gizeh Pyramids; these structures, the errors of which are but a few seconds of angle, agree in standing as much as 4′ or 5′ to the west of the present north.

Professor Newcomb some years ago, from observations of the transit of Mercury, concluded that the rotational period of the earth was not a fixed quantity, and it has since been amply shown from the study of the same phenomena that the period is subject to variation, increasing for a number of years and then diminishing again, and so on. I do not know whether any explanation has been offered of this phenomenon, but may it not indicate

movements of the viscous interior, more or less independent of that of the crust?

Some of the peculiarities of the distribution of temperature in the Tertiary seem to be more easily explained on the assumption of a geographical shifting of the pole, and as a slow shifting seems to be going on at the present moment, it may be looked upon as helping to solve the difficulty.

Mr. Marr says in his Address previously referred to that Dr. Neumayr in his work (Ueber Klimatische Zonen während der Jura und Kreidezeit) has, in the opinion of many geologists, established the existence of climatic zones in former times. This may be the best way of testing any supposed extensive shifting of the pole, although it is to be observed that up till the late Tertiary actual polar conditions must have been confined to a very few degrees round the pole, and may be, therefore, difficult to identify.

With regard to the possible geographical shifting of the axis, it has seemed to me that somewhat extensive changes could have taken place in former times when the earth was less rigid and the interior more closely resembling a fluid, in the following manner. We believe that the rotation of the earth is being slowly but surely retarded by the action of the tides. If the interior were fluid or thinly viscous, the retardation of the crust would not immediately affect the interior, as it would take time to communicate the retardation—that is to say, the interior would always rotate at a slightly greater speed than the crust. Now the solid crust would not be smooth underneath; if corrugations form exteriorily, through cooling or other causes, the under side would be roughened too. If the fluid or viscous interior were not absolutely homogeneous, and it is not likely ever to have been so, it would contain masses of solid matter, or of matter of at least firmer consistence than the rest. These floating masses on the under side of the crust would come in contact with the ridges, and would tend to produce—away from the equator—an acceleration at that spot which would cause the rotation of the whole to be modified and the axis shifted.

A general alteration of climate over the surface of the earth might be caused by an alteration in the constitution of the atmosphere. Mr. H. C. Russell at a meeting of the Royal Society of New South Wales in 1892 pointed out, when giving some particulars of probable life conditions on the Planet Mars, that the existence of a thin layer of olefiant gas in the atmosphere of this planet would allow the sun's heat to enter, but would prevent its radiation again into space, so that the existence of the addition of small quantities of such a gas if liberated by extensive volcanic disturbances from coal strata below would be the cause of materially raising the general temperature of the earth's surface. On the other hand, if the earth with the sun passed into regions of space which happened to be crowded with meteoric matter, the power of the sun's rays would be so much diminished that a considerable enlargement of the polar area and an extension of glacial phenomena into temperate regions would result.

In "The Climates of the Geological Past," Mr. Eugene Dubois shows how that in all ages up to the end of the Tertiary Period mild temperatures have been proved to exist up to within 10 or 15 degrees of the North Pole, and in the Eocene we have such in Grinell Land at 81¾° N., 95° W.; Spitzbergen 77½° to 79° N., about 20° E., while in the Island of New Siberia in latitude 75½° and 140° east longitude deposits of brown coal are found. In the southern hemisphere it has not been possible to penetrate so far, but in Kerguelen, which now has a rigorous climate, *Cupressoxylon* has been found, while at Punta Arenas, in the Straits of Magellan, 53½° S., the conditions appear to have been tropical. The author concurs with Heer in disputing the fact of any indication of geographical shifting of the pole, as the vegetation follows close on the pole all round, and if the ancient conditions seem to have been warmer on the Atlantic side, it is only similar to what is the case now. In the early Tertiary especially this intensity of conditions producing warmth might well have been even greater than now, as Europe consisted of islands and peninsulas, with inland seas and large bays, and there is little doubt that the Arctic Ocean was at that

time connected with the warm seas of Europe and Southern Asia. The author argues for a gradual cooling down of the sun as producing all the phenomena observable. The sun is now in the condition of a yellow star; all through the Palæozoic, Mesozoic and part of the Tertiary it was a white star, thus the heat conditions were more intense; and although the tropics need not have been hotter, the heat would be better distributed towards the poles. He points to the more ancient types of plants and animals (reptiles) as requiring warmer conditions, while warm blooded mammalia and birds are adapted to the cooler conditions now prevailing.

As a rule every writer looks to his own theory as being all-sufficient, whereas probably there has been a combination of conditions producing the effects, so that not only may we conclude that the reduction of the sun's radiating power may have had much to do with the present less favourable conditions, but that some of the intermediate changes may have been contributed to by various causes—namely, small shiftings in the geographical position of the earth's axis, increase in the eccentricity of the orbit, to some extent by an alteration of the distribution of land and water and the induced air and ocean currents, and also by cosmical causes and intercepting of the sun's heat by diffused interstellar matter.

Insular Floras and Oceanic Islands.

This subject is one the consideration of which cannot be separated from that of the permanence of oceanic beds.

Wallace divides islands into three classes:—Recent continental islands, ancient continental islands and oceanic islands, the latter being generally understood to be those surrounded by seas of more than 1000 fathoms, although as an exception it is acknowledged that some islands belong to the continental classes notwithstanding that the ocean barrier is now over 1000 fathoms. I think that Wallace scarcely sufficiently allows for the effect of long periods of time in altering depths. Time may be all that is

wanted to permit of a connection in the past of the remotest group of islands with the mainland.

There seems to be an argument in a circle as far as oceanic insular floras are concerned. First of all it is assumed that if the depth is over a certain amount—say, 1000 fathoms—former land connection was not possible; then comes the study of the flora and fauna of those islands which are thus situated, and those are then looked upon as characteristic of such islands—other islands have these characteristics—the conclusion is drawn that they also have never been connected with the land.

I shall not attempt to prove that important oceanic groups like the Sandwich Islands and the Galapagos Islands were once connected with any of the continental areas. I leave that to abler debaters than myself—like Captain Hutton and Dr. von Jhering—but I wish merely to draw attention to some of the difficulties that the holders of the oceanic insular theory have to contend with.

First let me say that there are many islands, formerly held to be oceanic islands, which are now acknowledged to have had a former continental connection—such as New Zealand, the Fiji and the Solomon Islands. Atolls and coral islands, and some islands of volcanic origin are probably acknowledged by every one to be truly oceanic, and about these there is no dispute. The difficulty lies in the determination whether such groups as the Samoan, Tongan, Marquesan and other groups of the Western and Central Pacific, the Sandwich Islands, Galapagos and some detached islands like Pitcairn and Easter Islands come under this category.

It is well known and acknowledged that there are about 200 species of plants the seeds of which stand immersion in salt water for a certain time, and are, therefore, capable of germination if thrown up by the sea on to a favourable spot, and out of these there is a smaller number which do not lose their germinating powers after prolonged immersion. Then, again, there are some seeds with a hard testa surrounded by pulp, which, after being eaten by birds, may be conveyed to islands at short distances, or perhaps for 50 or 100 miles, as the birds may be in the habit of

visiting them. There are also plants which have extremely light or small seeds, or, as in the case of most Compositæ, possessing a pappus, by means of which they are borne by the wind over long distances. Again, there are seeds with barbed hooks which may adhere to the feathers of birds, or others of small size produced by plants growing on the margin of water or elsewhere which may be taken up with particles of mud, and be thus conveyed over considerable distances. But when this list is exhausted there are still many plants growing on the larger islands the presence of which cannot be accounted for.

In the Hawaiian or Sandwich Islands, according to the late Dr. Hillebrand's investigations, there are 999 species of phanerogams and vascular cryptogams. After deducting from this number the usual littoral and drift species, and many useful and ornamental plants probably introduced by the natives, and even allowing a margin for endemic evolution of new species, after introduction of those from elsewhere, it must be acknowledged that a great power of belief is required to satisfy one that the balance are all introduced.

The situation of the islands is this:—They are 2,040 miles from the coast of America, 1,860 from the Marquesas, and 2,190 miles from Tahiti. It can be seen how small a chance there is for winds, waves and birds to bring together the component parts of this rich flora from elsewhere. There are a few plants representative of the Eastern Australian Region, a few with Southern South American affinities. Most of the plants are allied to those on the nearest coast of America. Dr. Hillebrand divides the flora according to the zones which they inhabit on the islands. These are as follows:—

1. Lowland zone.
2. Lower forest zone—1,000 or 2,000 feet.
3. Middle ,, —up to 5,000 to 6,000 feet, this being the most luxuriant.
4. Upper forest zone—up to 8,000 or 9,000. On Mauna Kea shrubby vegetation extends to 11,000 feet.

5. Bog flora of high table land of Kauai, and of the broad top of Mt. Ecka or West Maui. Here are representatives from Antarctica (New Zealand, Falkland Islands, Southern Andes, &c.).

It is to be noted that there are 40 endemic or peculiar genera, one of which is the curious Lobeliaceous tree *Sclerotheca*.

It is most difficult to understand how winds, waves and birds could have combined to bring the seeds of all these plants together and pop them down just on the right spot where germination could take place.

The Galapagos Islands are another example; but here the distance from the mainland is much less, and the number of species smaller, so that the possibility of accidental introduction is largely increased, but it is curious that the different islands possess different species, and those chiefly distinct from the mainland. This remark applies to the land snails as well as the plants.* The affinities of the endemic flora are entirely American. A few plants such as *Lipochæta laricifolia*, have congeners in the Sandwich Islands, and not in America, but the arboreous Lobeliaceæ are absent. There are only five species noticed common to all islands, two species in four islands, and six in three, according to Mr. Botting Hemsley's account in the "Botany" of the Challenger. If species have drifted from the mainland, or been conveyed by birds or otherwise, why should the same species not have been conveyed to all islands, or those on one island not have been transferred to the others?

The floras of the larger islands of the south-western Pacific have a decidedly Malayan character, and there is not the development of endemic genera which would lead to the certain conclusion that the islands were relics of a former more extensive land area.

In the "Botany" of the Challenger Expedition, p. 68, there is an interesting and instructive remark on the Flora of the Eastern

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* See Mr. Dall's paper in the Proc. Acad. Nat. Sci. Philadelphia, 1896, p. 395.

Pacific Islands, which runs thus:—"The Australasian genus *Metrosideros* penetrates as far eastward as Pitcairn, where, as in the Sandwich Islands, it forms large woods; and the prominence of such other Australasian or Asiatic genera in the Sandwich Islands as *Pittosporum*, *Alphitonia*, *Cyathodes*, *Scævola* and *Cyrtandra* is noteworthy. On the other hand, the peculiar Sandwich Island types seem to have had a former wider extension, as is indicated by the Lobeliaceous arboreous genus *Sclerotheca* and a species of *Phyllostegia* in Tahiti."

When treating of Tristan d'Acunha in the South Atlantic, Mr. Botting Hemsley says (Appendix, p. 313):—"Whether the present distribution of *Phylica nitida* was brought about by the agency of birds is highly problematical. The distribution of the genus, like that of many others of the African region, points rather to a former greater land connection."

The scientific methods of the present age, starting with Darwin and Wallace, have been chiefly directed towards discrediting the miraculous and catastrophal, and towards accounting for all phenomena by means of existing mechanical causes. The old method of explaining facts is admittedly unscientific, but are we not tempted under modern methods to press the argument just a little too far the other way; and having found, for instance, that some plants, and even some animals, can be dispersed by winds, waves, birds, &c., assume that all have arrived on the scene by the same group of chances? Is it unscientific to assume the existence in the past of larger land areas in the Pacific and elsewhere than now exist?

Captain Hutton says:—"In the distribution of reptiles and of some birds in Polynesia, we have evidence of the existence of a former continent. The brush turkeys or megapodes are birds that are unable to fly, and yet they are found in Borneo, Celebes, the Philippine Islands, Australia, New Guinea, New Caledonia, the Marion Islands, the Samoan Islands and others in the Pacific. Reptiles are widely spread throughout the islands of Polynesia, and we can only account for it by supposing a former land communication. Mr. Wallace, in his 'Island Life,' attempts to

explain the fact by suggesting that reptiles have some unknown and exceptional powers of dispersal. But if so, why is the phenomenon limited to Polynesia? And why should Mr. Wallace himself explain the small number of reptiles in Great Britain and Ireland by the supposition that they are unable to cross the English and Irish Channels?"*

The results of the Challenger dredgings seem to show that the principal part of the Pacific was ocean during the Tertiary period, but it is not impossible that chains of volcanic islands or masses of land may have existed during or before that period and that these, being of a shifting character, at first connected with a continent and afterwards cut off, might preserve the relics of a continental fauna and flora. A continent properly so called can scarcely have existed. The difficulties are too great in the way of such a supposition, but only connections similar to that which we are certain existed between New Zealand, New Caledonia, the Fijis, and the main land which was perhaps at its period of greatest development in a state of oscillation need be conceded.

Captain Hutton's theory of a bridge for the migration of marsupials to Patagonia across the Pacific presents too many difficulties, and my remarks above are by no means intended to support the idea, for the absence of relics on the road is a strong argument against it. Neither on the islands nor on the mainland of Asia between Europe and the Malay Peninsula have at present any fossil remains been found of those animals which alike are represented in Tertiary Europe and Patagonia.

The facts seem rather to point to the conclusion that the Australian Marsupials were derived either from an ancient and extended Patagonia or that the ancestors in both countries were developed previously in some Antarctic region now submerged.

Some light on the subject of the former distribution of land and water is thrown by Dr. H. von Jhering, who has kindly

* New Zealand Journal of Science, Vol. i. 1883, p. 411.

furnished me with a copy of his Treatise "Das neutropische Florengebiet und seine Geschichte," (Engler's Jahrbuch, 1893). This able treatise deals with matters of special interest to us, and therefore deserves notice in this place, but I find that I have not space to refer to it at the length which it deserves, and I must therefore now confine myself to stating his main arguments, at the same time recommending those interested to study the original work. The author sets himself to upset Wallace's axiom of the permanence of continents and oceans which would, if true, require that South America was always cut off from connection with south-eastern Asia as it is at present, and he disputes the validity of the assertion that the bottoms of oceans over 1000 fathoms in depth could never have been dry land. He says that greater depths only indicate longer time for subsidence. The effect of separation at different epochs would be that we should find the fauna limited to the groups which had reached their development before then, and he points to the Pacific Islands, where the tertiary fauna are absent altogether, as proof of their isolation in Mesozoic times, while on the other hand lizards, ancient types of mollusks and insects are found.

The author divides South America into three regions. The northernmost has affinities with North America, the middle one with Africa, Madagascar and Bengal. These regions he suggests after an investigation of the fresh water fauna. They were in the Cretaceous and early Tertiary separated by ocean. He concludes that a great continent which he calls "Archhelenis" extended across the Atlantic to Africa and beyond; this possessed probably no mammalia, but a rich fresh water fauna and identical reptiles and amphibia. The lower region he calls "Archiplata," it was formerly connected with New Zealand, and partly with Australia and Tasmania. The early Tertiary mammals existed in this region, but not in Archhelenis. The Dasyure group connects with Australia. The *Anoplotheridæ* and *Therodomyidæ* have affinities with the Eocene fauna of the old world. Argentina can only have received her Eocene mammals across antarctic lands. In the Pliocene North and South America

became united, and an interchange of forms thereafter took place. The land connecting La Plata and Patagonia with South Eastern Asia he calls "Archinotis." He says the bridge between South America and Africa broke up before that between India and Africa, so that when the middle and southern South American regions became united no neotropical African types could migrate to Australia.

The author then discusses the various methods by which plants and animals are understood to be transported across the ocean, and throws doubt upon the whole theory of oceanic islands. Speaking of the island group of Ferdinand Noronha, he says, "It is certain that on the main island birds scatter the seeds of berries, fruits, &c., but when wind and birds do not cause the spread of the plants even from one island to another the distance of a gunshot, how can one believe that this means of distribution is effective across gaps of hundreds or thousands of kilometers?" The author disputes the fact of the Andean migration; he says there is not a species common to the Californian Sierra Nevada and the Andes. With regard to the exchange of plants of higher latitudes north and south of the equator, he is of opinion that formerly these must have been capable of existing in warm regions as well as in cold. Even now *Ranunculus*, *Polygonum*, *Stellaria media*, *Samolus Valerandi*, *Veronica anagallis*, *Parietaria debilis*, &c., are not sensitive to climate. He says that formerly plants were not so restricted by climate, so that the following genera are found together in the Upper Pliocene of Niederrad and Höchst am Main: *Juglans*, *Aesculus*, *Carya*, *Liquidamber*, *Corylus avellana*, *Betula alba*, *Picea vulgaris*, and the alpine *Pinus cembra* and *Pinus montana*. The author then discusses the distribution of various genera, *Podocarpus* and other southern *Coniferæ*, *Cocos*, *Nipa* and other Palms, *Cupuliferæ*, &c. He is of opinion that the completeness of the Indo-Australian territory must have been longer retained than the connection of Australia and New Zealand, and he says that if the genera *Canis* and *Sus*, the *Muridæ*, &c., could push into New Guinea and Australia, the connection with Asia must have lasted into the Miocene. During the whole

Tertiary period there was a constant change of mammals between North America and Europe, but it was not complete; probably those that could not face a temperate climate could not pass. This might explain the fact of the *Anoplotheridæ* and *Theridomyidæ* being found in the Argentine beds and Europe but not in North America. The author then discusses the fresh water flora and finds the conclusion derived from their consideration to fit in with that deduced from the fresh water fauna.

The South American Mammalia—Recent and Extinct

I cannot conclude my Address without making special reference to the wonderful discoveries of fossil mammals recently made in South America. The importance of these discoveries to us is that in this region not only placental mammals of very peculiar types have been found differing in important respects from allied forms in other parts of the world, but that marsupials of distinctly Australian affinities also occur. Here I should like to refer to a most interesting find in Ecuador of a living animal of a strange type, and the proof that it is marsupial in character. Before this the only living representative in America was the Opossum (that is the true Opossum or Didelphys which belongs to the Polyprotodont group). This new animal called *Cœnolestes* resembles the group of Kangaroos and Australian Opossums (properly called Phalangers) in being diprotodont, but differs from them in not being syndactylous.

The work of describing the fossil mammals is being carried out by Lydekker and Osborne; Scott and F. Ameghino have also written on the subject. I do not propose, however, to go into details, which indeed would be premature, seeing that the whole subject has only been partially investigated, and I would rather refer to the works of the above authors. I wish, however, to call attention to what appears to be the latest deliverance on this subject by Florentino Ameghino, which has been translated by Arthur Smith Woodward, and published in the Geological Magazine for January of this year under the title—"Notes on the Geology and Palæontology of Argentina." This is a very important

paper, because if the views as to the age of the beds and the affinities of the remains are corroborated, Patagonia must have been a centre of distribution of mammals not only for the Antarctic regions of the time, but also for Europe and, perhaps, North America.

Mr. F. Ameghino shows that beds exist—red sandstones—containing remains of Dinosaurs and undoubtedly of Upper Cretaceous Age. Above those and quite continuous with them comes the Pyrotherium Formation, containing armoured and unarmoured Edentates, peculiar Carnivora, Plagiaulacidæ, Hystricomorphous Rodentia, peculiar Ungulates and primitive forms of Primates. Ameghino includes *Pyrotherium* among the Ungulates, and considers it allied to the Proboscidea, but Woodward asks in a note at the end whether it may not be allied rather to *Diprotodon*. Ameghino says that if these beds are not Cretaceous, then Dinosaurs lived in Patagonia until a more recent epoch than in other portions of the globe.

Above the Pyrotherium Formation comes the Patagonian Formation, which has been erroneously confounded with the marine formations of Parana. The mollusca of the Patagonian Formation have been stated by D'Orbigny, Sowerby, Philippi, Hupé, Remond de Corbineau and Steinman to be partly of Eocene and partly of Upper Cretaceous Age. The objection to this antiquity is the presence of remains of Cetacea, which only appear in Europe during the Miocene, but F. Ameghino thinks the group might well have originated earlier in the southern hemisphere, and says their remains are more primitive in type, as has been recognised by Lydekker.

Next above comes the Santa Cruz Formation, which was at one time supposed to be anterior to the Patagonian, on account of the latter having been confused with the Parana. There are here numerous remains of extinct mammals, gigantic birds and reptiles. There are marsupials of the Diprotodont group, which like the living *Cænolestes* above referred to, and unlike the Kangaroos, are not syndactylous. These are stated to resemble

the Plagiaulacidæ. This formation comprises a marvellous collection of animals including *Homunculus*. Philippi considers it to be of Miocene Age.

Above this lies the Boulder or Tchuelche Formation, which, as Darwin has shown, is of marine not glacial origin. This is stated to be of Miocene Age.

Later signs of geological phenomena are the transverse valleys of Patagonia and the Pampean Formation, which latter contains six or seven successive mammalian faunas. Dr. von Jhering says of the mollusca that almost all the species live still on the shores of Brazil.

There are numerous plant remains in the beds included in this formation, and it is to be hoped that investigation of the same may be made without delay.

Tertiary Plant Remains in Australia.

Mr. T. S. Hall and Mr. G. B. Pritchard have done much to unravel the difficulties of determination of the age of the Tertiary beds of Victoria.

Much confusion had previously resulted from a misunderstanding of the position of what is termed the Older Basalt, which was considered Miocene by Professor McCoy, on account of its being supposed to overlie beds of Miocene Age. Messrs. Hall and Pritchard have shown this view to be erroneous, and the date, instead of being Miocene, to be early Tertiary,* as it has been found to be overlapped by acknowledged marine Eocene strata.

Underneath the Lower Basalt lie in various localities of the colony indurated clays cemented with ferruginous or siliceous material and containing beautifully preserved plant remains, and the conclusion seems almost forced upon us that these are Upper Cretaceous in age.

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* On the Age of certain Plant-bearing beds in Victoria. Aust. Assoc. 1893, Adelaide.

Messrs. Hall and Pritchard in the same paper suggest that the beds at Dalton and Vegetable Creek, which have the same lithological character, and which Baron Ettingshausen considered Eocene, may have to be referred back to the Cretaceous also.

Messrs. Hall and Pritchard have written several valuable papers discussing the age of the Tertiary strata of Victoria, and Mr. T. H. Wright has in the most painstaking manner investigated the geological features of an area of Gippsland, and proved the true sequence of the beds, in some cases entirely reversing previously received ideas. Unfortunately I am unable through lack of time and space to enter into these matters as I should like, and can, therefore, only refer to the papers read by those gentlemen before the Royal Society of Victoria and Australasian Association, and in the case of Mr. Wright's investigation, to the 8th Report of the Geological Survey of Victoria.

Earliest Dicotyledons in the Northern Hemisphere.

In the Report of the United States Geological Survey (Vol. xvi. Part 1), just received, there is a paper by Professor Lester F. Ward entitled "Some Analogies in the Lower Cretaceous of Europe and America."

Up to 1888 the oldest known dicotyledon was one from the Middle Cretaceous of Greenland, which was described by Heer under the name of *Populus primaeva.*

Professor Fontaine in 1888 found in some of the Lower Potomac Series, in what was supposed to be Jurassic, some portions of leaves resembling dicotyledons, but not easily distinguishable from the lower groups, ferns, cycads and other gymnosperms.

In the Report to which reference is now made Professor Ward says:—"On numerous occasions, dating as far back as 1878, I have expressed the opinion that the dicotyledons could not have had their origin later than the Middle Jura, and it will not surprise me if the final verdict of science shall place the Potomac formation, at least the lower member, in which the plants occur, with that geologic system."

Since then the known flora of the Potomac formation has been greatly increased by further discoveries, and an unbroken series from the oldest to the newest beds brought to light—in the latter the dicotyledonous element largely predominates.

Marquis Saporta called attention not long after Prof. Fontaine's discovery to the existence of peculiar forms in the Lower Cretaceous of Portugal, some of which he referred to his group of Proangiosperms while others represented true Dicotyledons. These beds are probably of the age of the Gault, that is Middle Cretaceous. It was found that other collections from older beds also contained dicotyledons, and in 1891 Saporta published a paper on the subject.

Professor Ward, comparing the Jurassic flora of Portugal with the Potomac beds, concludes as follows:—"But the special interest which these comparisons have in this place is the intimate bond which they furnish between the late Jurassic of Portugal (supposed to correspond closely with the Kimmeridge Clays of England, but perhaps running up into the Portland beds and thus closely approaching the Purbeck, which has been treated in this paper as part of the Wealden) and the oldest Cretaceous of America, which some geologists in this country make to extend some distance into the Jurassic, but which is here treated as a Cretaceous deposit."

Earliest Dicotyledons in Australia.

The fossils of the Oxley beds are well developed dicotyledons, quite equal in development to those found in the Upper Cretaceous in Europe and North America. The Oxley beds are near the top of the Ipswich Coal Measures, which are supposed to be at latest Jurassic in age. The difficulty of reconciling the fact of the full development of the dicotyledonous type in Australia with the very archaic rudimentary types of the same age in North America which are mentioned by Lester Ward, struck me very forcibly, and as in the western parts of the Colony it had been shown that the Lower Cretaceous beds lie, conformably, or at an angle not distinguishable, upon the beds below them, I thought

it desirable to inquire of Mr. R. L Jack whether it was not possible that the same condition existed on the coast side of the Dividing Range, and that thus the beds in question might really be of Lower Cretaceous Age.

Mr. Jack's reply is as follows:—"I cannot see my way to putting the Oxley beds on a higher horizon than the rest of the Ipswich formation. Stratigraphically it would not work. They form an integral part of the formation which from top to bottom yields the assemblage of plants on which the Triasso-Jurassic age of the whole was founded. They are pretty well up in the series, but what evidence there is is all against their being the uppermost part or anywhere near it. I believe them to be *below* the thick Murphy's Creek Sandstone and the Clifton Coals and Shales which give the same fossil plants as the shales associated with the coal seams of Ipswich proper."

If Mr. Jack's views as to the age of the beds is correct, they point undoubtedly to the conclusion that at an age when European and American dicotyledons exhibited a rudimentary or transition character, the southern hemisphere already possessed types of high development. Before this becomes an accepted fact, it is needless to say that some further corroboration of the conclusions as to the correspondence in age of the so-called Jurassic beds of Australia and those of the northern hemisphere should be sought.

I wish to take this opportunity of expressing my best thanks to Messrs. R. Etheridge, Junr., T. W. E. David, E. F. Pittman, R. L. Jack, T. S. Hall, G. B. Pritchard, J. H. Wright, H. C. Russell, C. J. Merfield, C. Hedley, R. T. Baker, H. C. L. Anderson, J. J. Fletcher and others for the assistance they have given me in the preparation of this Address and that of last year by placing books and facts at my disposal.

On the motion of Professor Haswell, seconded by Mr. W. S. Dun, a very hearty vote of thanks was accorded to the President for his interesting Address.

The subjoined financial statement for the year ending March 31st, 1897, was presented by the Hon. Treasurer, and adopted.